

is being produced at the center by thermonuclear reactions. Now, this represents one of the possibilities for testing the turbulent transport from the center to the surface. And just from orders of magnitude we also obtain a turbulent diffusion coefficient $d \sim 10^3$.

C^{13} is also interesting because if we take the earth abundance ratio $C^{12}/C^{13} \sim 80$, do we observe in the sun the same or possibly a smaller ratio? This cannot be considered as settled. Suppose $C^{12}/C^{13} > 80$ can be explained by C^{13} burning at the center of the sun because the C^{12}/C^{13} ratio in the carbon cycle is about 4. This is an increase and seems to go the other way around, but we have to remember that the carbon is essentially turned into nitrogen during the carbon cycle, which means finally the destruction of carbon in favor of nitrogen and consequently a greater destruction of C^{13} than C^{12} . If the ratio is larger than 80, this could possibly give an indication of the presence of turbulent transport from the center to the surface of the sun. I don't mean at all that this is a demonstration which has taken place because as you can judge, there are a number of difficulties concerning the initial abundances which are present.

COMMENTS

A. Ingersoll I want to discuss the question of whether the oblateness measurements that *Dicke and Goldenberg* [1967] made do indicate that the core of the sun is rotating rapidly, or whether there is an equally attractive alternate possibility. Dicke and Goldenberg looked at the shape of the sun in visible light, and there are really three ways that the sun might look oblate in visible light. The first possibility is that the equipotentials, gravitational plus centrifugal, are oblate, which would be the case if the interior of the sun were rotating rapidly. The second and third are variations of the possibility that the solar equator is somehow hotter than the poles. If the equator were hotter, it would also be brighter, and this might be confused with an oblateness because of the limitations of seeing in the earth's atmosphere.

I divide this hotter-equator possibility into two categories because the first of these, the one considered and rejected by Dicke and Goldenberg, is that the equator of the sun is hotter at all depths by a certain amount of ΔT . This would be like saying that the equivalent temperature of the sun is greater at the equator than it is at the poles, or that the radiant flux is greater at the equator than it is at the poles. Their measurements suggest that this is an unlikely possibility, although I do not feel that it can be conclusively ruled out.

The second possibility, which Spiegel and I have proposed [*Ingersoll and Spiegel*, 1971], is that the equator of the sun is hotter only in the chromosphere but not in the photosphere. This possibility is much easier to confuse with a real oblateness. To show why this is so, I must digress to define certain aspects of the Dicke-Goldenberg experiment. They took an image of the sun and projected it onto a perfectly circular occulting disk, slightly smaller than the solar image. The radial angular distance from the edge of the disk to the mean solar limb is δ , and they did their experiments for $\delta \approx 6.5''$, $12.8''$, and $19.1''$. In each case, they scanned around the edge of the disk, measuring all the light that was coming from beyond the occulting disk, and looked for an increase in flux at the equator relative to that at the poles. This difference in flux is the signal they used to infer the solar oblateness. The important thing about this quantity δ is that for each of the three possibilities that I mentioned earlier, there is a different relationship between signal amplitude and δ .

First, if the sun is truly oblate, then the signal is approximately independent of how much sun is in the field of view, and therefore, the signal amplitude is proportional to δ^0 — that is, independent of δ . In this case the signal simply depends on the difference between the equatorial and polar radii of the sun, and not on how much sun is occulted. Next, if the equivalent temperature of the sun is greater at the equator than at the poles, then the signal amplitude is proportional to the fraction of the solar disk in the field of view — that is, to δ^1 . From the data taken at the three values of δ , Dicke and Goldenberg

concluded that this was very unlikely. What Spiegel and I pointed out is that if the equator is hotter than the poles, but only in an optically thin part of the sun's atmosphere, then the dependence on δ is intermediate between these two and is proportional to $\delta^{1/2}$. Here we postulate an equatorial temperature excess in parts of the sun's atmosphere that can be seen even on the extreme limb — that is, in the very top of the photosphere and in the chromosphere. In this case, each emitter in the field of view contributes as much to the signal as any other, and the number of emitters in the field of view is simply proportional to the solar surface area exposed from the edge of the occulting disk to the limb, and this is proportional to $\delta^{1/2}$.

Figure 1 is our reworking of the Dicke and Goldenberg data. We have plotted signal

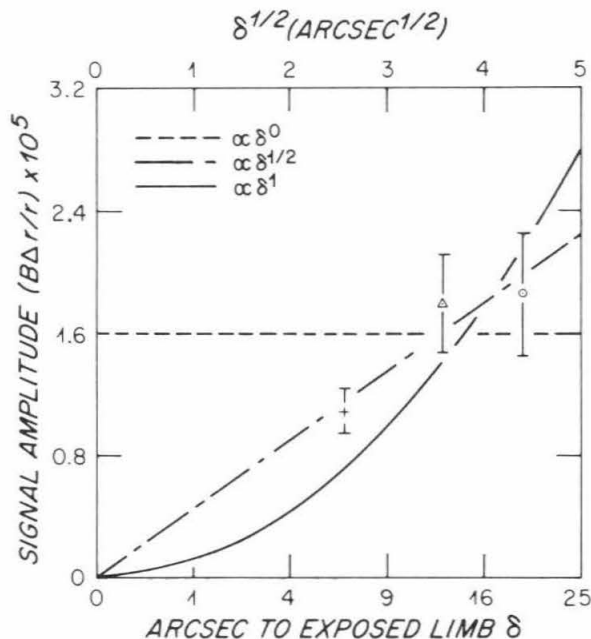


Figure 1 Signal amplitude versus $\delta^{1/2}$ after correction for surface rotation. Units are $B\Delta r/r$, where B is relative brightness at the occulting disk, and $\Delta r/r$ is measured oblateness [Dicke and Goldenberg, 1967]. Error bars give the square root of variance for each δ . Curves illustrate three possible dependences on δ .

amplitude versus $\delta^{1/2}$, for $\delta = 6.5''$, $12.8''$, and $19.1''$, which are the three values of δ used in the experiments. The three lines drawn represent the three possibilities: signal amplitude $\propto \delta^0$, $\delta^{1/2}$, δ^1 . Actually, the signal due to a true oblateness would not be exactly $\propto \delta^0$, but would depend on the brightness at the edge of the occulting disk, and this brightness increases slightly with δ . So a true oblateness is consistent with these data. Dicke and Goldenberg ruled out the parabola, signal $\propto \delta^1$. The curve shown corresponds to $\Delta T_e \approx 5^\circ \text{ K}$ — that is, to a 5° excess in the equivalent temperature of the sun at the equator relative to that at the poles. Obviously, it would be very interesting to measure that somehow — I suppose by sending a satellite over the poles. The line on the graph labeled $\delta^{1/2}$ corresponds to what Spiegel and I suggested, with

$$\tau_0 \Delta T \approx 0.3^\circ \text{ K}, \quad \tau_0 \ll 0.1$$

Here ΔT is the required temperature difference between equator and poles, which is restricted, we assume, to an optically thin layer. And τ_0 is the value of the optical depth at the level below which this temperature difference is assumed to vanish. The restriction $\tau_0 \ll 0.1$ simply ensures that this layer is optically thin. Examination of figure 1 shows that this possibility fits the Dicke and Goldenberg data quite well.

Now if Spiegel and I are correct in our interpretation, and if the chromosphere really is hotter at the equator than it is at the poles, the heat source for the equatorial chromosphere must be greater than the heat source for the polar chromosphere by a specific amount. This excess mechanical flux upward at the equator must be whatever is necessary to supply the excess emission implied by the relation $\tau_0 \Delta T \approx 0.3^\circ \text{ K}$. The required excess flux is $\Delta F \approx 2.5 \times 10^7 \text{ ergs/cm}^2/\text{sec}$, which is comparable to what many people believe is the total mechanical and hydromagnetic energy flux into the chromosphere. So if our interpretation is correct, then we have to be prepared either for a mechanical heating of the chromosphere, which is larger than what most people believe, or a variation in this heating from equator to pole, which is comparable in magnitude to the heating itself.

REFERENCES

- Dicke, R. H.; and Goldenberg, H. N.: Solar Oblateness and General Relativity. *Phys. Rev. Ltrs.*, Vol. 18, 1967, p. 313.
 Ingersoll, A.; and Spiegel, E.: Temperature Variation and the Solar Oblateness. *Ap. J.*, Vol. 163, 1971, p. 375.

DISCUSSION *R. H. Dicke* There are three points I would make. First, the question was raised as to whether a general temperature difference of the photosphere between the equator and the pole could account for the observations. The measurements were made with three different amounts of limbs exposed, which lead to a light flux ratio of approximately 1.0 to 2.5 between the smallest and the greatest amount. Under an oblate sun hypothesis these two signals have a ratio of about 1.0 to 1.2 and when we renormalize (correct the signal of the biggest exposure by a factor of 1.2 downward), the observations are satisfactory. I can't believe that they would be satisfactory if we had reduced the signal by a factor 2.5. There would then be a sizable discrepancy in those three curves. I don't think that's possible.

On the question of a hot layer, I think one must go far above an optical depth of 0.1 to make the scheme work. For levels above 0.01 you need at least a 40° temperature difference between the equator and the pole. For this case, I think that the signal could be sufficiently close to what we observed that this might be a satisfactory way of accounting for the signals. On the other hand, one has to make a physically reasonable statement. There are two requirements to be satisfied. One is the requirement of energy balance for the necessary steady state – the problem of getting excess energy at the equator into the particular layer, the upper photosphere, to heat it up enough to give the excess radiation. And the other requirement is one of dynamic balance for the necessary steady state. There may be several ways this can be done; the one that's been suggested by the authors, which is to require that the angular velocity increase outward in the upper photopshere with a scale height of about 1,500 km, may well be in difficulty with what is known observationally about the rotation of the sun at various levels. So I would say that insofar as the observations are concerned it is possible that one could account for them in this way, but I haven't seen a coherent physical statement of how such a physical state would be maintained or dynamically balanced.

A. Ingersoll The first point Dicke raised was that he didn't feel that the data could be consistent with a temperature difference between equator and poles that extended

deep into the atmosphere of the sun. Now, that really hinges on whether you feel that the parabola can be made to fit the three data points, the parabola being the solid line in the graph I showed earlier.

R. Dicke I don't know how you got these points. The paper didn't list them — the paper didn't even give the normalization ratios that you would have had to know to compute these points; the ratios weren't in the paper.

A. Ingersoll We assumed that the values of δ and the values of the photospheric brightness at the edge of the occulting disk were those which you gave in your paper. We used the limb darkening curve you gave in your paper —

R. Dicke We didn't give a limb darkening curve.

A. Ingersoll Well, not in *Dicke and Goldenberg* [*Phys. Rev. Letters*, 18, 31, 1967], but in *Dicke* [*Ap. J.*, 159, 1, 1970] from which we took these values.

R. Dicke But those were not observations, but a theoretical limb darkening curve from a theoretical paper.

A. Ingersoll Let me put it this way: All the data we got for making this graph came from various papers you have written; we consulted no others for this.

Now, the second point, I guess, was the question of the dynamical balance. If we are to accept the fact that the parabola does not fit the data, then the temperature difference between the equator and pole is concentrated only in the chromosphere, and it is true that you need to balance the forces implied by this horizontal temperature difference. The most likely way is that angular velocity should be increasing with height. We calculate that if angular velocity increases by ~ 5 percent in 100 km over some 100-km region near the temperature minimum, that would be enough. So there's another observation that should be made in order to test this observation.

E. Schatzman There is a very well-known solar oblateness in the meter wavelength that corresponds to a structure of the corona, but very high in the corona. The oblateness is considerable. So might there be a relation between your assumption concerning the chromosphere and what has been observed at meter wavelength?

R. H. Dicke It seems to me that the postulate of the increasing angular velocity does fit observations; that is, one sees angular velocity increase with height in the chromosphere. The sign is correct for the chromosphere and consequently may be correct for the upper photosphere where the balance is actually needed if the upper photosphere is to be extended on the equator with a higher temperature. So it's not a question of whether the idea is qualitatively wrong but whether in fact it is quantitatively right. (Ed. note: See comment by Livingston, p. 304).

COMMENTS

C. P. Sonett We have carried out extensive calculations regarding a mechanism for early electrical heating of meteorite parent bodies with the view to obtaining clues about the early solar system especially the question of the pristine solar spin rate and evolving conditions in the solar nebula just after condensation of the primary objects. The proposed mechanism and the calculations which have been carried out are based upon the following observational evidence. Certain classes of meteorites, particularly the iron-nickels and achondrites, has been exhaustively studied for evidence of cooling from elevated temperatures [*Wood*, 1964; *Goldstein and Short*, 1967]. The iron-nickels show evidence for cooling rates which range approximately from $1\text{--}10^\circ/\text{million years}$ indicating that at the time of the cooling cycle these objects were at depths within parent bodies to several hundred km radius. Some error might accrue in these estimates on the basis that for the nickel-irons the diffusion of Ni across grain boundaries between kamacite and taenite, both of which are Ni-Fe phases, varies from the values used because of "doping" of the matrix by trace elements which can adversely affect diffusion coefficients. However,